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Geochronology, Stratigraphy, and Typology

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A new and conceptually different tool is coming into use more frequently in the literature of vertebrate paleontology. I refer to the increased availability of radiometric dates for rocks containing or associated with vertebrate fossils. This is a most welcome and exciting addition to the science because it adds the possibility of new information that could provide a time matrix within which comparative rates of evolution could be determined. In addition, centers of origin and directions of migration could be plotted. Such information is not at hand presently, but hopefully it will come.

At the symposium of the Society of Vertebrate Paleontology — Geological Society of America in 1973, considerable data was presented to establish this matrix. A beginning was made with the distribution of correlation charts, each with a radiometric scale, for parts of the Tertiary in selected areas of North America. These kinds of charts are new. Earlier ones for Tertiary continental correlation were either without such a scale (Wood et al., 1941); or with a scale but based on "approximations" (Simpson, 1947) rather than laboratory-supplied data points; or with a scale but only for local areas (Wilson et al., 1968; Emery, 1973; Smedes and Prostka, 1972). The early basic work of Evernden et al. (1964) was essentially a test of the temporal succession of North American Land-Mammal Ages by means of isolated dates. A wide geographic coverage of samples for short segments of time was not yet available.

Although the charts distributed at the Dallas meeting are to be regarded as preliminary, some were meant to accompany an Abstract (McKenna et al., 1973; Tedford et al., 1973) which was

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published in the "Abstracts with Programs" of the Geological Society of America. Some of the individual charts or abstracts presented rather radical revisions of the temporal span of some "North American Provincial Ages." At the same meeting there was a notable lack of uniformity in stratigraphic nomenclature, a situation some find to be of no consequence but others find rather distressing. With the advent of new techniques involving a new kind of time-scale, I believe it is necessary to make an attempt to emphasize the importance of stratigraphic terminology used in vertebrate paleontology. Previous attempts to do so have been made and are summarized in the excellent paper by Tedford (1970), but he did not deal with the nomenclatural problems caused by the appearance of radiometric dates. Neither did I in earlier papers that dealt with stratigraphic practice in vertebrate paleontology (Wilson, 1959, 1971). I was involved, however, in a proposal (Bell et al., 1961) for a classification of stratigraphic units that involved the radiometric time-scale.

A geologist, stratigrapher, or vertebrate paleontologist working in the field must deal with three things: rocks, fossils, and breaks (bedding planes and erosion surfaces). For some vertebrate paleontologists only the fossils are important and the rock is something to be removed in order to study the morphology of the specimen. For other vertebrate paleontologists, the rock is important because it may provide a clue to the environment in which the animal died, perhaps how it died, and may even furnish information on the environment in which the animal lived. The local layers of rock verify the relationship earlier than or later than for the contained fossils. Erosion surfaces and bedding planes may be useful for interpreting environmental relationships of the body of rock in which the specimens were contained and also provide evidence for the passage of time. The correct interpretation of the cut-and-fill relationship of channels in continental sediments is extremely important for determining "earlier than" or "later than" when small increments of time are involved.

At various times all three of these observable things — rocks, breaks, and fossils — have been used either singly or in combination as a basis for stratigraphic classification and for the interpretation of the passage of time. Observation of the accumulation of rock and the resulting superposition were probably first used by Steno (1669). Observation of the removal of rock and the resulting erosion surface go back at least to Hutton (1795). The use of fossils by Smith in the

1790's depended on the observation of the superpositional relationship of suites of fossils. To him similar fossils meant similar lithology as well as similar time. However, it was not until after 1865 that the interpretation of the passage of time could be made from the evolutionary position attained by plants or animals.

Werner (1787) provided a classification based on kinds of rocks. Chamberlin (1898) proposed a classification based on unconformities. Sedgwick (1838) (Paleozoic) and Phillips (1840) (Mesozoic and Cenozoic) gave a classification based on life forms. The latter classification was based on the observation of the superposition of faunas in stratified rock. Each of these classifications has been used for a time, either singly or in combination.

Throughout the history of application of the various classifications in stratigraphy, there has been a parallelism with methodology in the application of the biological classification. The typologic school in stratigraphy has been the dominant one and, strangely to me at least, is currently being reinforced by the so-called biological stratigraphers.

Tedford (1970) clearly reviews the evolution of attempts to establish stratigraphic subdivisions for the continental Tertiary deposits of North America. I should like to show how attempts developed within the framework of the evolution of stratigraphic classification on a national and international scale. The formally-adopted codes for stratigraphic classification date back to the International Geologic Congress held in Bologna in 1881. Two hierarchies of terms were set up, one for rocks: Group, System, Series, Stage; and a second for time: Era, Period, Epoch, Age. The hierarchies were to operate in parallel. Each rock category was equated to a time category that was the span of time necessary to accumulate that particular unit of rock (System-Period). This is sometimes called a dual classification in reference to the two hierarchies. In 1900 the Paris Congress added Zone and Phase.

Group	Era
System	Period
Series	Epoch
Stage	Age
Zone	Phase

Zone was a body of rock characterized by fossils and Phase was the time during which the animals or plants lived and when the rock of the zone was accumulated. This was still a dual classification in the sense that there were only two hierarchies, but a totally different criterion for the recognition of one of the categories within one of the hierarchies was introduced. Rock units were the basis for construction of the units Group (now Erathem), System, Series, and Stage, whereas fossils were the basis for the construction of Zones. The dual classification still consisted of two hierarchies, but two different sets of criteria, rock (for formation) and fossil (for Zone) were used to construct units within the "rock" hierarchy. The term Group is now used in a very different sense, and Phase is seldom used at all.

The two hierarchies with some slight modifications became the basis for world-wide stratigraphic classification, but a rather sharp contrast in method arose as a result of the inclusion of the two criteria, rocks and fossils, within the rock hierarchy. European statigraphers emphasize the fossils and North American stratigraphers emphasize the rocks. Rocks were adopted for cartographic purposes by the United States Geological Survey, and formation was established as the fundamental unit. "The discrimination of sedimentary formations shall be based upon the local sequence of the rocks, lines of separation being drawn at points in the stratigraphic column where lithologic characters change . . ."(Walcott, 1903, p. 23). But further on, on the same page, is the statement, "The formation should be recognized and should be called by the same name as far as it can be traced and identified by means of its lithologic character, its stratigraphic association, and *its contained fossils*." (Italics mine) The emphasis is on lithology, but a concession is made to fossils.

The two hierarchies were the established classifications available to the early vertebrate paleontologists. Figure 1 of Tedford (1970) reflects that the so-called "rock" hierarchy also included fossils. It was implicit, nonetheless, within the dual hierarchy classification that a unit, whether based on lithology, e.g., White River Formation, or based on fossils, e.g., *Equus* beds or *Eohippus* Faunal Zone, has an equivalent parallel time unit. A comprehensive list of authors that used the two hierarchies is found in Schenck and Muller (1941, Table 2). The two hierarchies were the only ones recognized by Ashley et al. (1933), and it was the one to which the Wood Committee (1941) attempted to conform.

The year 1941 also saw the publication of the succinct paper by Schenck and Muller that broke the long-standing dominance of the two-hierarchy classification. In this paper the "lithogenetic terms" were divorced from the two original hierarchies and set up as a third (Group, Formation, Member). "In order to focus attention upon the fact that the lithogenetic units do not necessarily bear any direct relation to the units in columns I [Time] and II [Time-stratigraphy], they are set off and written in such a form as to preclude any fixed notion of a correlation." The ice was broken; rock units were no longer equated with time unit.

Following World War II there was an enormous increase in the study of near-shore marine ecology. This was done primarily by teams of stratigraphers, paleontologists, and biologists working for the research branches of large oil companies. The research teams had modern field equipment for coring and underwater mapping and, in addition, the laboratory facilities for radiometric dating and sample analysis. The various published reports of this work furnished substantial data that many marine near-shore animals were closely tied to their environment, and as the environment moved, so moved the animals. This, of course, was not a new concept, but a population of biostratigraphers worked with sufficient sophistication over a large enough area and with a thick enough section of sediment to be able to demonstrate that a thickness of rock characterized by certain suites of fossils could be time-transgressive. In the United States this population of biostratigraphers was able to incorporate into the 1961 A.C.S.N. Code a new stratigraphic unit — a body of rock strata characterized by its content of fossils and divorced from any time unit or time-rock unit. It "is defined solely by the fossils it contains, without reference to lithology, inferred environment, or concepts or time" (A.C.S.N., 1972, Art. 20b).

Unfortunately, the name Zone was used for this unit. I say unfortunately because Zone had attained through usage, particularly in Europe, a time or time-stratigraphic connotation. Such a usage was thought to be justified by such statements as, "Since the fossils usually serve as our nearest approach to time-markers, it follows that the units of this standard column are stratal units defined by time." (Schenck and Muller, 1941, p. 1420). These two authors were able to divorce the formation from time but not the zone. That separation has to wait until after 1961 when the revised A.C.S.N. Code appeared, and whether or not it will be adopted in the International Guide of the I.S.S.C. is being warmly debated.

The Wood Committee during the late 1930's and early 1940's had only the "official"or "formal" framework of the two hierarchy classification. The hierarchy of rock units was not useful for their purpose, and neither was Zone. They state, "A tacit provincial time scale has already arisen, but its efficiency is seriously hampered by the confusing use of identical terms for rock units and for the time units which are implicitly but not explicitly generalized from them . . ." (Wood, et al., 1941, pp. 2-3). The usage of Zone was objectionable because of "the vicissitudes of using zoological nomenclature." And so like others before them (d'Orbigny, Buckman, Oppel, Williams), they attempted to set up time units based on fossil assemblages. I strongly disagree with Savage (1962), Tedford (1970), and Tedford et al., (1973), all of whom claim that the Wood Committee typified some of the North American Ages by the geochron of a rock stratigraphic unit. This gives a typologic interpretation to the Wood Committee report that I believe was not intended.

The Wood Committee (1941) time-scale and correlation chart has been widely used, and severely criticized. It attempted to set up a succession of units of "purely temporal significance." But in the process, according to Savage (1962, p. 55), "fourteen of the eighteen ages were typified by lithostratigraphic units," and according to Tedford (1970, p. 687), "In practice, this resulted in the definition of segments of time corresponding to the limits of lithostratigraphic units (for example, Arikareean equals the temporal span of the Arikaree Group)." Such an interpretation is a strictly typological interpretation in stratigraphy and is comparable to a typological interpretation in biology (Simpson, 1940, 1961) wherein the type specimen defines the species rather than serving as the name-bearer and part of the hypodigm. I cannot prove but I believe that the Wood Committee did not mean that their ages were meant to be interpreted typologically and have the same temporal span as the lithostratigraphic unit that furnished the name. As evidence, I present their own words (Wood et al., 1941):

> "That is, they are meant to cover all of Tertiary time without reference to whether each year, century, or millennium is represented by a known faunule or stratum. The type of each age necessarily belongs to it, and the sequence and *approximate* (italics mine) scope of the ages are thus intended to be definitely fixed (barring discoveries which should lead to radically different interpretations). *However, the ages are not necessarily coextensive with their types, and the precise limits between successive ages are intended to be somewhat flexible* and may presumably be modified in the light of later

discoveries. Thus the Wasatchian age is more extensive than the known mammalian faunas of the type Wasatch and probably less extensive than the time equivalent of the entire Wasatch group in the type area."

Furthermore, from conversations with E. H. Colbert, a member of that committee, I have learned that the types were intended to be based on faunal assemblages and not the geochron of a particular lithologic unit.

Most lithostratigraphic units were found to be time-transgressive, and their definition was divorced from time in the 1961 Code, but the concept of a geochron persists (Savage, 1962, and others that claim the Wood Committee defined the North American Provincial Ages on, for example, the geochron of the Arikaree Group).

The Wood Committee did not expect a person working outside of Nebraska to be able to recognize Arikareean on any other basis than the comparison of the unknown fossils to the known *published* fauna from the type area. The Wood Committee intended to set up biochrons with flexible limits. The taxonomic value of the geochron is only local, particularly in trying to deal with problems of the stratigraphy and paleontology of continental deposits. A geochron is not a useful unit for stratigraphic classification; its abandonment by the U.S.G.S. for the Precambrian part of the geologic time-scale is an apt illustration (James, 1972).

The taxonomic value of the biochron in stratigraphic classification depends on the cliche "similar fossils mean similar time." This is applicable only if there is no outside evidence to the contrary. If there is evidence to the contrary, like transgression, the fossils are said to be environment indicators and time-transgressive. One seldom-mentioned characteristic of the ordinal scale is that if one cannot tell the relationship earlier than or later than, the events were simultaneous. This prevents the recognition of area of origin and direction of migration.

The Wood Committee of the 1941 report worked long and hard. They produced a document that attempted to follow the stratigraphic rules of the Ashley Report of 1933. The Wood Committee report has proved useful to the vertebrate paleontologist of North America, and although its definitions have been reinterpreted by some, it has provided a working nomenclature for 30-plus years. At one time it was hoped that the Wood Committee report could be updated at approximately ten-year intervals by adding onto the framework they had provided, but attempts to revise the Wood Committee report stalled. The radiometric dates now add another complication to the already supercomplicated stratigraphic classification. The dates provide for the first time a ratio scale (Stevens, 1946), whereas all previous scales (including a magnetic polarity scale) are ordinal. A ratio scale has equal intervals (years) and a fixed zero (Present, arbitrarily 1950), whereas an ordinal scale has no fixed zero, no equal intervals, and gives only the relationship earlier than or later than.

Where does this leave us? What does have good time-taxonomic value for the continental Tertiary of North America? I do not think there is a continent-wide panacea, but the best hope so far is a matrix of radiometric dates. Within the limits of error of the method used, this will enable us to determine time on a metric scale at an increasing number of points. Hopefully, these points can be associated with the ordinal stratigraphic successions of vertebrate faunas. But let us avoid the pitfall of tying faunas or North American Provincial Ages to radiometric ages — they must be independent. The radiometric-date matrix is a metric scale and must be the background onto which points or the ordinal scale of superposition of both rock and faunas can be plotted. If we project an isochronous plane out from the base of the type section of the Arikareean on the local date of 29 m.y., let us be prepared to find elements of the Arikareean fauna (perhaps major ones) that date at 31 or 32 m.y. and elements of a Whiteyan fauna younger than 29 m.y. Catastrophism is still extinct — theoretically, at least. It is only by freeing ourselves from equating lithostratigraphic and biostratigraphic units with time units that we can extricate ourselves from a classificatory mess. I hope a future Tertiary correlation committee would not define time units on anything but radiometric dates. Let homotaxical units, faunas (and zones), and lithologic units (formations) remain within their own ordinal scale. They are four-dimensional topologic spaces and can logically be conceived as such.

Hopefully, a future correlation committee can overcome two other difficulties. The first is the nomenclatural difference we have with our invertebrate colleagues. They use a biologic name in association with Zone whereas we have preferred a geographic name. This is a matter of nomenclatural techniques and could be resolved. More importantly, this leads to the second, closely related difficulty widespread among vertebrate paleontologists, that is the lack of recognition of a biostratigraphic unit. Perhaps part of it is because of the use of the word "body" in the definition of biostratigraphic units in both the A.C.S.N. Code and the I.S.S.C. Guide. I do not think either of these documents intends that the word "body" be taken in its literal singular sense. The *Olenellus* Zone is certainly not a single *body* of rock. But everywhere *Olenellus* is found, it is in rock, and all the rocks it characterizes constitute the *Olenellus* Zone. The abandonment of the term Zone by the vertebrate paleontologists left a vacuum. Contrary to Tedford (1970), who disagreed with me, I still firmly believe that the Garvin Gully Fauna and others of the Texas Gulf Coastal Plain are biostratigraphic units. I made no claim that they were "geologically contemporaneous"; the eastern end may be older or younger than the western end of any one of them. My main interest was with the faunas, and my interest in the rock was as an indicator of environment. The kind of rock is useful information in discovering new localities and perforce in trying to straighten out a nomenclatural mess that involved the recognition of lithologic units. It is possible to go for one hundred miles or more on the Texas Gulf Coastal Plain and collect from a succession of rocks (call them channel sands, back-levee clays, fluviatile deposits, calclithites – it is not important for the definition), *Parahippus* and an associated fauna, and in superposition *Merychippus* and an associated fauna. The three-dimensional bodies of rocks (whether they are channels, crevass splay, ox-bow lake deposits, etc.) that are characterized by a particular set of fossil animals constitutes a biostratigraphic unit. The fossils do not occur anywhere else but in the rock. To speak of a Garvin Gully Fauna or a *Parahippus* Zone for the Texas Gulf coastal area, to me, at least, conveys the same information, and neither one necessarily implies an association of geologically contemporaneous species. I wish I knew whether the Garvin Gully Fauna migrated from Mexico or Florida or Nebraska or originated on the Gulf Coast. The only hope of learning this will be from an independent time measure.

Correlation charts and codes improve over the years, and by clearly separating the formal rock stratigraphic units from time units and the formal biostratigraphic or faunal units from time units can we hopefully speed up the understanding. Long live T. H. Huxley and homotaxis.

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